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published in
Journal of Experimental Psychology: Human Perception and Performance
2001

DOI (link to publisher)
10.1037//0096-1523.27.5.1019

document version
Publisher's PDF, also known as Version of record

Journal of Experimental Psychology:
Human Perception and Performance
2001, Vol. 27, No. 5, 1019–1038

0096-1523/01/$5.00 DOI: 10.1037//0096-1523.27.5.1019

Learning to Visually Perceive the Relative Mass of Colliding Balls in Globally and Locally Constrained Task Ecologies

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Novice observers differ from each other in the kinematic variables they use for the perception of kinetic properties, but they converge on more useful variables after practice with feedback. The colliding-balls paradigm was used to investigate how the convergence depends on the relations between the candidate variables and the to-be-perceived property, relative mass. Experiment 1 showed that observers do not change in the variables they use if the variables with which they start allow accurate performance. Experiment 2 showed that, at least for some observers, convergence can be facilitated by reducing the correlations between commonly used nonspecifying variables and relative mass but not by keeping those variables constant. Experiments 3a and 3b further demonstrated that observers learn not to rely on a particular nonspecifying variable if the correlation between that variable and relative mass is reduced.

Research has shown that humans can visually perceive kinetic properties of their environments such as the weight of lifted boxes (Runeson & Frykholm, 1983), the peak force exerted by bimanual pullers (Michaels & de Vries, 1998), and the relative mass of colliding balls (Runeson, 1995). Given that the ambient optic array comprises only kinematic variables (e.g., velocities and angles), kinematic variables must form a basis for the perception of kinetic properties.

The bulk of research on the visual perception of kinetic properties has been carried out in the colliding-balls paradigm. In this paradigm, observers are asked to judge which of two colliding balls is the heavier or, more recently (Jacobs, Michaels, & Runeson, 2000), to make quantitative estimates of mass ratios. Runeson (1995) showed that the mass ratio of colliding balls is specified by, among other kinematic patterns, the relative amount of velocity change. That is, he showed that $m_B/m_A = |v_A - u_A|/|v_B - u_B|$, where m_A and m_B are the masses of the two balls, u_A and u_B are the velocities of the balls before impact, and v_A and v_B are the velocities of the balls after impact (see Figure 1 later). Depending on the particular stimulus set, other kinematic variables might also correlate highly with the mass ratios. Such nonspecifying variables include the difference in exit speeds and the difference in scatter angles; an exit speed is a ball's speed after the moment of impact, and a scatter angle is the angle between a ball's velocity before and after impact.[1]

Whereas a debate has revolved around whether perceivers can or cannot use variables that specify kinetic properties (Gilden & Proffitt, 1989, 1994; Proffitt & Gilden, 1989; Runeson, 1995; Runeson & Vedeler, 1993), more recent evidence suggests that novice perceivers often use nonspecifying variables and possibly graduate to the use of specifying variables after practice (Jacobs & Michaels, 2001; Jacobs et al., 2000; Michaels & de Vries, 1998; Runeson, Juslin, & Olsson, 2000). For example, participants in a study conducted by Michaels and de Vries (1998) were trained with feedback to estimate the maximal force exerted by standing humans or stick figures, who briefly pulled a bimanually gripped handle. Observers' judgments correlated highly with force, and more so after training with feedback. Regression analyses revealed that observers differed in the kinematic variables they used and that they often changed from reliance on a simple variable to reliance on its derivative or a compound variable. In one of the experiments of Michaels and de Vries, feedback was given on kinematic variables that did not specify pulling force, and participants flexibly converged on variables that satisfied the implicit task demands created by the feedback.

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We thank the Netherlands Organization for Scientific Research (NWO) for funding this project. The research was conducted while David Jacobs was supported by Grant 575-12-070 from the NWO Foundation for Behavioural and Educational Sciences that was awarded to Claire Michaels. Sverker Runeson's participation and his laboratory resources were supported by the Swedish Council for Research in the Humanities and Social Sciences.

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[1] Note that we use velocity in the sense of a vector with both a length and a direction. Speed is used as a scalar, namely, as the length of a velocity vector.

Kinematic patterns that specify to-be-perceived kinetic properties always satisfy the task demands because global constraints guarantee a one-to-one relation between these patterns and the specified properties. Among global constraints are included natural laws and other boundary conditions that are invariant over all settings. Also, in a particular task ecology, otherwise nonspecifying patterns might be related one to one to kinematic properties because of (arbitrary) local constraints (Runeson, 1989; e.g., in the collision paradigm, a lack of variation in precollision velocities; see subsequent discussion). The kinematic variables on which participants in the Michaels and de Vries (1998) study came to rely after practice often were nonspecifying patterns that were highly useful only in a particular collection of displays; other display collections with other levels of kinematic–kinetic correlations led to the use of other variables.

Jacobs et al. (2000) reported a similar process of convergence on the more useful variables. That study addressed the issue of perceptual learning with observers who practiced, with feedback, making quantitative judgments of the mass ratios of colliding balls. In a pretest, observers seemed to use the difference in exit speed or a combination of exit-speed and scatter-angle differences. After training, some observers came to detect a mass-specifying variable, whereas other observers came to rely on the combination of the exit-speed and scatter-angle differences. For the remaining observers, these two possibilities could not be distinguished. Why did so many observers continue to rely on the combination of the differences in scatter angle and exit speed while mass-specifying variables were available? In the set of displays used, the combination allowed for quite accurate performance because it correlated highly (r = .93) with the to-be-perceived mass ratios. The small errors caused by the less-than-perfect correlation might have been difficult to distinguish from random errors caused by, for instance, the finite resolution of the perceptual system. Observers might have continued to rely on the combination because it allowed reasonably satisfactory feedback in the set of collisions used in practice.

In the present study, we investigated how relations between candidate kinematic variables and mass ratios affect the variables on which observers come to rely. Observers were trained to make quantitative estimates of the mass ratios of colliding balls using different sets of collisions. We analyzed which kinematic patterns accounted for the resulting systematic variance in the judgments. The question addressed in Experiment 1 was whether observers change in the variables on which they rely if all candidate kinematic variables correlate perfectly with the to-be-perceived mass ratios and hence allow accurate performance and satisfactory feedback. We hypothesize that, in such a situation, observers continue to rely on the variables with which they start and do not discover a specifying variable. In Experiments 2 and 3, on the other hand, we explored conditions that might facilitate convergence on specifying variables.

Experiment 1

The purpose of Experiment 1 was twofold. First, we wanted to replicate the findings of Jacobs et al. (2000) with a different set of collisions. Second, we wanted to determine whether observers also change in the variables on which they rely if all candidate kinematic variables correlate perfectly with the to-be-perceived mass ratios. To test this, we compared two groups trained on stimulus arrays in which the family of correlations between the kinematic variables and the mass ratios differed. The groups are referred to as the *global-constraint group* and the *local-constraint group;* global constraints refer to constraints that apply to all sets of collisions, and local constraints refer to boundary conditions that are specific to a particular set of collisions. Identical pretests and posttests permitted assessment of the effects of training.

The pretest, global-constraint, and posttest conditions used the same precollision velocities as the experiment of Jacobs et al. (2000), although with a narrower range of mass ratios. The correlations among the kinematic variables that follow from these precollision velocities and the mass ratios simulated in the different sets of collisions are presented in Table 1. The specifying invariant correlated perfectly with the simulated mass ratios. The correlations between the candidate nonspecifying variables and the specifying invariant (i.e., the simulated mass ratios) were all less than perfect.

The local-constraint group was trained with a specially assembled set of collisions with less variation in the precollision velocities. In this set of collisions, all candidate kinematic variables correlated perfectly with the to-be-perceived mass ratios (see Table 1). Reliance on any of these variables might lead to accurate performance and positive feedback. We hypothesized that participants in the local-constraint group who began with reliance on differences in exit speed or scatter angle would continue to rely on these kinematic variables throughout the experiment.

Method

Participants. Sixteen students from the University of Uppsala were assigned randomly to the two groups.

Table 1
Correlations Between the Kinematic Variables in Experiment 1

Variable	1	2	3	4
Test phases				
1. Specifying invariant[a]	—	.37	.79	.84
2. Exit-speed difference		—	.08	.44
3. Scatter-angle difference			—	.93
4. Exit-speed and scatter-angle difference[b]				—
Global-constraint practice				
1. Specifying invariant[a]	—	.44	.83	.89
2. Exit-speed difference		—	.16	.50
3. Scatter-angle difference			—	.94
4. Exit-speed and scatter-angle difference[b]				—
Local-constraint practice				
1. Specifying invariant[a]	—	.99	1.00	1.00
2. Exit-speed difference		—	1.00	.99
3. Scatter-angle difference			—	1.00
4. Exit-speed and scatter-angle difference[b]				—

[a] Correlates perfectly with the simulated mass ratios. [b] We used the linear combinations of the differences in exit speed and scatter angle that best predicted the simulated mass ratios in the to-be-considered sets of collisions to calculate the correlations shown.

Apparatus. The collisions were simulated with a one-of-a-kind analog computer system (Runeson & Vedeler, 1993). A digital computer controlled the experimental variables and sequenced the phases of trials and sessions: setup, collision display, response recording, feedback, pauses, and so forth. The collisions were displayed on a vertically oriented ground-glass circular screen with a 17.7° visual angle diameter. This screen was viewed through a collimator lens system that permitted free head movements without changing the angular properties of the display. An alphanumeric response monitor that could show two rows of 20 characters was placed below the collision display.

Design. The experiment consisted of a 64-trial pretest without feedback, two 74-trial blocks of training with feedback, and a 64-trial posttest without feedback. The computer system simulated collisions using predefined mass ratios and precollision velocities. Eight mass ratios that differed by equal logarithmic steps from 1:3 to 3:1 were used in the pretest and posttest. Nine mass ratios were used during training; they differed by equal logarithmic steps from 1:4 to 4:1. Participants were told that the range of mass ratios simulated in the test phases and in practice could be different.

Figure 1 shows that the precollision velocities can be decomposed in collision and sweep components. In the sets of collisions simulated in the pretest, global-constraint training, and posttest, two pairs of collision components were crossed with two pairs of sweep components to form four pairs of precollision velocities. The collision components were −13 and 38 mm/s (−1.6°/s and 4.6°/s) or 13 and 64 mm/s (1.6°/s and 7.7°/s). The sweep components of the balls with the higher and lower collision components were, respectively, 38 mm/s (4.6°/s) and 15 mm/s (1.8°/s) or 15 mm/s (1.8°/s) and 38 mm/s (4.6°/s), always in opposite directions.

In the set of collisions simulated in the local-constraint practice, collision components were always 26 mm/s (3.2°/s), in opposite directions. The sweep components were also 26 mm/s (3.2°/s), again in opposite directions. Because the precollision velocities were identical in the different collisions, the exit speeds and scatter angles of the balls depended only on the mass ratios.

A dotted or continuous outline was randomly assigned to two circles 13.5 mm (1.7°) in diameter. The circles collided at 9 mm (1.1°) left or right

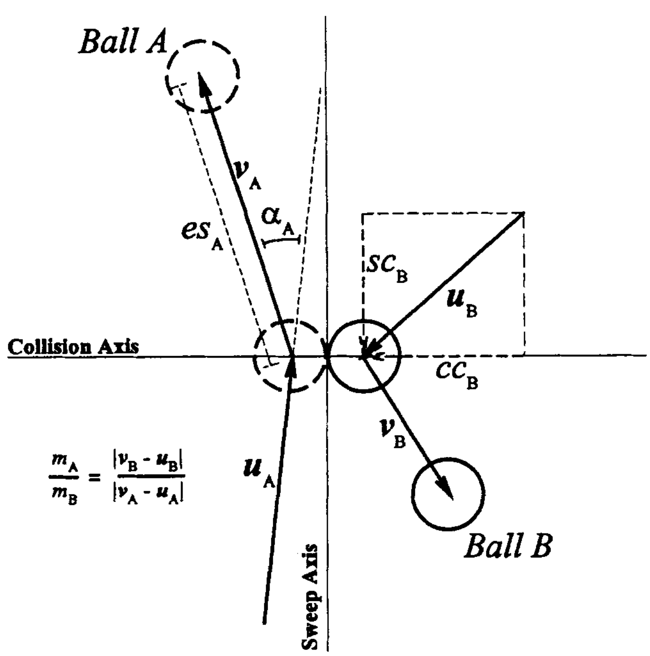

Figure 1. Two-dimensional collision with the exit speed (*es*) and scatter angle (α) of Ball A and the collision (*cc*) and sweep (*sc*) components of the precollision velocity of Ball B.

and 9 mm (1.1°) above or below the center of the screen, randomly chosen. This increased the variation in the simulated collisions and unconfounded the difference in exit speeds from the difference in the moments at which the balls reached the edge of the round screen. The display was mirror reversed and rotated in steps of 30°, also randomly. The random variation in the collisions reduced the chance that identical collisions were presented and thereby virtually ruled out the possibility that observers would recognize individual collisions. The collisions had a restitution factor of .9, which is about the highest restitution of real-world collisions.

Procedure. The experiment was divided into two sessions. The first session started with participants reading written instructions and completing eight instruction trials without feedback. During these first few trials, the experimenter explained and demonstrated the response devices, after which the participant gradually took over. The instruction trials were followed by the pretest and the first practice block. The second session comprised the second practice block and the posttest. The sessions lasted 1 to 1.5 hr each and were usually carried out on consecutive days. Mandatory pauses of at least 5 min were included after the pretest and after the second practice block. In addition, optional pauses were suggested every sixth trial.

Participants started a series of six trials by pushing a button. After the presentation of a trial, 41 answer alternatives could be chosen. They differed by equal logarithmic steps from 1:10 to 10:1. Mass ratio estimates were entered by turning a knob that moved a marker along the upper row of the alphanumerical display. The dialed ratio also appeared as a numerical ratio (e.g., 2.4:1) in the middle of the bottom row of this display. A chosen ratio was confirmed by pushing a ready button; in the test phases, this also initiated a new trial.

On practice trials, confirmation of a judgment was followed by visual and auditory feedback. The marker on the upper row of the alphanumerical display jumped from the position of the participant's estimate to the position closest to the simulated ratio. The simulated ratio was also shown numerically in the middle of the alphanumerical display. Accurate judgments (judgments that differed from the simulated ratio by less than 0.45 natural log units) were followed by a low beep and two high beeps, fair judgments (judgments that differed from the simulated ratio by more than 0.45 but less than 0.9 log units) were followed by a high beep, and incorrect judgments (judgments that differed from the simulated ratio by more than 0.9 log units) were followed by a low beep. Participants were paid according to their performance. Throughout the experiment, accurate judgments were rewarded with 1.5 SEK (about $0.18) and fair judgments with 0.75 SEK.

Results and Discussion

We first present analyses that address observers' abilities to perceive relative mass and then try to reveal the kinematic variables on which such mass perception was based. Mass ratio was defined as the mass of the dotted ball divided by the mass of the continuous ball (see Jacobs et al., 2000, Footnote 4, for the motivation to use this definition instead of others). Furthermore, all analyses were performed on the logarithms of the judged and simulated ratios, because the logarithms form a symmetrical scale around the 1:1 ratio.

Figure 2 presents the judgments averaged per simulated mass ratio and per group in the pretest (left panel) and posttest (right panel). All averaged judgments seem to approximate the simulated mass ratios, which suggests that participants were able to perceive the mass ratios. This conclusion was confirmed by within-subject Pearson product–moment correlations between the judgments and the simulated mass ratios. The squares of these correlations are presented in Figures 3 and 4 (solid circles). All but 3 of the 64 correlations differed significantly ($p < .05$) from zero; variation in judgments coincided with variation in simulated mass ratios.

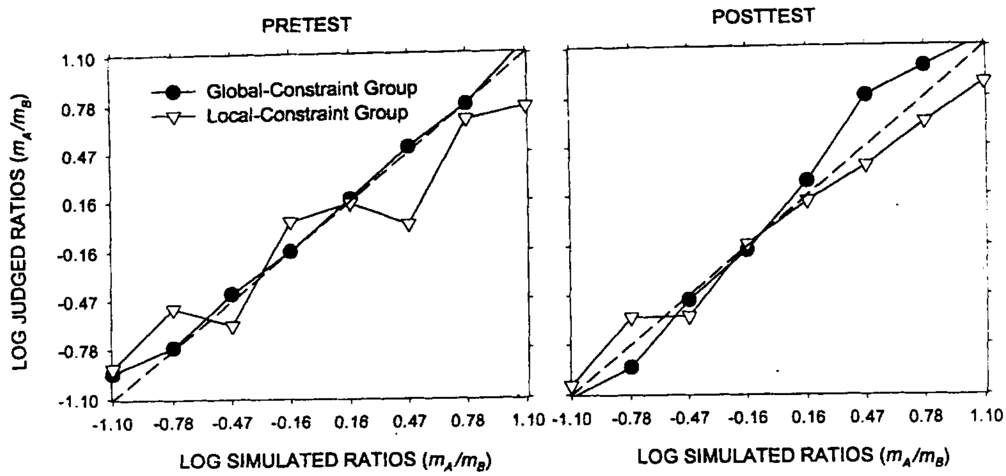

Figure 2. Averaged logarithms of judged mass ratios as a function of the logarithms of the simulated mass ratios in the pretest (left) and posttest (right) for both groups in Experiment 1. The simulated mass of Ball A (the dotted circle) is referred to as m_A, and the simulated mass of Ball B (the continuous circle) is referred to as m_B.

We performed an analysis of variance (ANOVA) on these correlations with group (global vs. local) as a between-subjects variable and test phase (pretest vs. posttest) as a within-subject variable.[2] A significant effect of phase, $F(1, 14) = 15.4, p < .01$, indicated that the observers performed better after practice than before. The effect of group, $F(1, 14) = 2.5, p > .10$, and the interaction, $F(1, 14) = 1.3, p > .10$, were not significant. Apparently, observers did not perform better in one of the groups, and neither improved more after one type of practice. Nevertheless, an analysis of covariance on the correlations in the posttest, with group (global vs. local) as a between-subjects variable and the correlations in the pretest as a covariate, indicated that observers performed better after global-constraint practice, $F(1, 13) = 4.8$, $p < .05$. This means that if we factor out the large performance differences in the pretest, individuals in the global-constraint group performed better on average on the posttest than those in the local-constraint group.

The significant correlations between the judgments and the simulated mass ratios might reflect the use of a specifying variable or the use of variables that were merely correlated with the mass ratios. Recall that, in the sets of collisions used in the pretest and posttest, several nonspecifying kinematic variables correlated with the simulated mass ratios (see Table 1). To determine whether participants relied on kinematic variables other than mass-specifying information, we computed zero-order correlations between judgments and exit-speed differences and between judgments and scatter-angle differences. We also computed, for each block of trials and each observer, the multiple correlation of the judgments regressed against both the exit-speed and scatter-angle differences.

Figure 3 presents the squares of the correlations for participants in the global-constraint group. The judgments of Observers 1 to 6 correlated most highly either with a specifying variable (solid circles) or with a combination of the exit-speed and scatter-angle differences (open triangles) throughout the experiment.[3] The correlations nevertheless suggested differences among the strategies of these observers. For instance, Observer 1 seemed to change from reliance on a combination to reliance on a specifying variable; Observer 2 seemed to rely on a specifying variable in all blocks; and Observer 3 seemed to rely on a combination in all blocks.

The pretest judgments of Observers 7 and 8 correlated most highly with exit-speed difference (open diamonds) and a combination of exit-speed and scatter-angle differences. The combination did not appear to be a better predictor, which implies that the difference in exit speed was used in this block by these observers. Because the difference in exit speed correlated only moderately with the to-be-perceived mass ratios ($r = .37$ for Blocks 1 and 4 and $r = .44$ for Blocks 2 and 3), this strategy yielded poor judgments. Perhaps because of the discouraging feedback, Observer 7 came to rely on a combination in the latter blocks. Similarly, Observer 8 changed her strategy and came to rely on a specifying variable or on a combination, although it does not seem that she could sustain her performance without feedback. In sum, observers in the global-constraint group differed and changed in the variables they used. In the final blocks, all observers appeared to rely on a combination of exit-speed and scatter-angle differences or on a specifying variable. This replicates the findings of Jacobs et al. (2000).

We now turn to the more important Figure 4, which presents the squares of the correlations for the local-constraint group. The trivial consequence of the perfect correlations among variables is obvious in Blocks 2 and 3. The interesting question is whether observers used the same variables in the pretest and posttest; most observers did so. In the pretest and posttest, the judgments of

[2] All tests on correlations were done on the z transformations of the correlations: $z_r = ½[\ln(1 + r) - \ln(1 - r)]$.

[3] One might wonder whether the differences in Figures 3 and 4 are statistically significant. We conducted statistical tests on all pairwise comparisons of correlations within each block of trials for each observer, but we have not presented these results to avoid cluttering the figures. Instead, we present more global tests showing that the main findings are significant. In addition, in Footnote 4 we briefly address a few correlations that are crucial for our hypotheses.

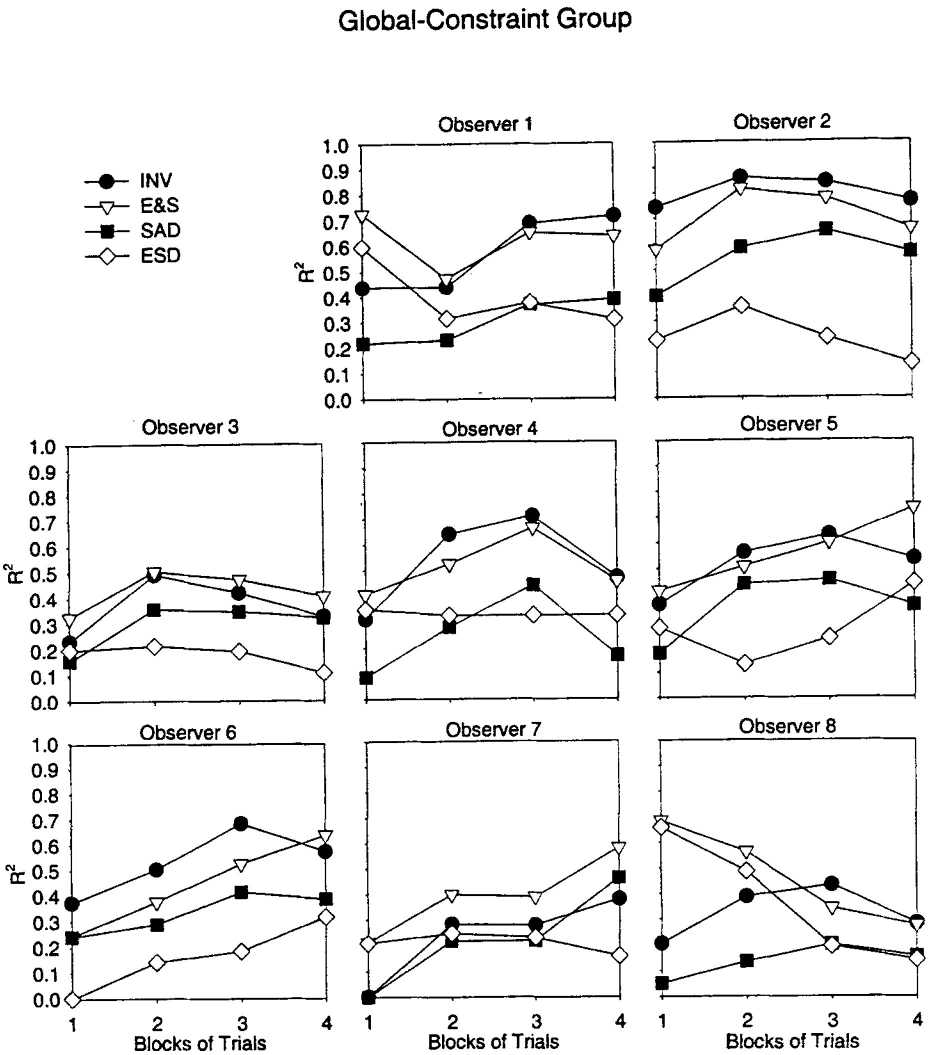

Figure 3. Squares of correlations between the various kinematic variables and judgments of observers in the global-constraint group of Experiment 1: pretest (Block 1), practice (Blocks 2 and 3), and posttest (Block 4). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

Observers 1, 4, and 8 correlated most highly with a specifying variable or with a combination. Observer 3 seemed to rely on a specifying variable in the pretest and posttest. Observers 2, 5, 6, and 7 appeared to use exit-speed difference in the pretest and continued to do so in the posttest.

Together, Figures 3 and 4 seemed to confirm our hypotheses. Participants in the local-constraint group who relied on exit-speed difference in the pretest continued to rely on exit-speed difference in the posttest. Participants in the global-constraint group, on the other hand, came to rely on mass-specifying information or on the combination in the posttest.[4]

In agreement with this conclusion, the posttest judgments of participants in the global-constraint group correlated more highly with relative mass than with exit-speed difference (r_{mean} = .72 vs. r_{mean} = .49), whereas the posttest judgments of participants in the local-constraint group correlated less highly with relative mass than with exit-speed difference (r_{mean} = .57 vs. r_{mean} = .69). A *t*

[4] As indicated in Footnote 3, here we illustrate some of the statistical tests between the correlations that are crucial for our hypothesis. Observers 2, 5, 6, and 7 of the local-constraint group were claimed to rely on the exit-speed difference in both the pretest and the posttest. According to a *t* test for independent correlations (Bruning & Kintz, 1987), the differences between the exit-speed correlation and the invariant correlation and the differences between the exit-speed correlation and the scatter-angle correlation were significant ($p < .05$) for Observers 2, 5, and 6 in the pretest and for all 4 observers in the posttest.

Observers 7 and 8 of the global-constraint group were claimed to rely on exit-speed difference in the pretests and on a specifying variable or a combination later in the experiment. For both observers, the differences between the exit-speed correlation and the invariant correlation and the differences between the exit-speed correlation and the scatter-angle correlation were significant ($p < .05$) in the pretest. The judgments of Observer 7 correlated significantly higher with the combination than with any other variable in the posttest. For Observer 8, the differences were not significant in the posttest. Thus, the main conclusions seem to be reliable, although not all of the differences between the correlations were significant.

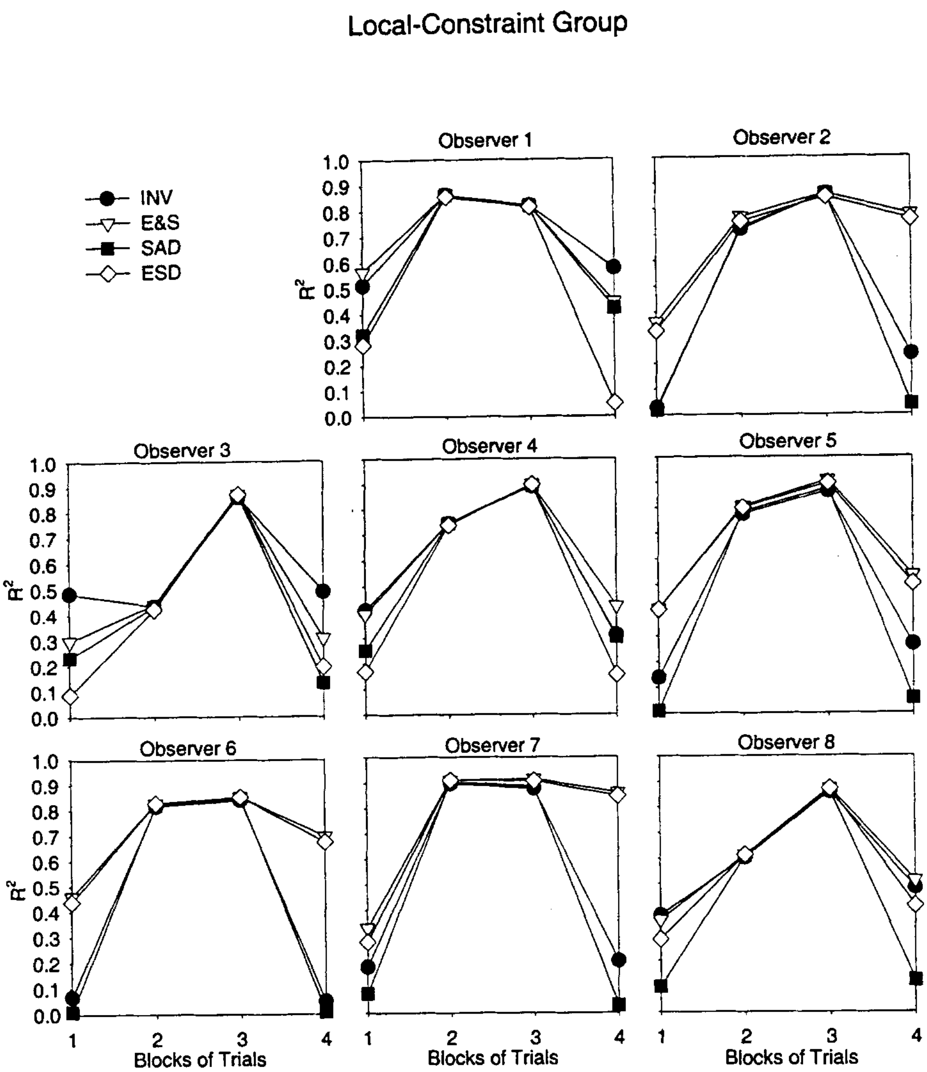

Figure 4. Squares of correlations between the various kinematic variables and judgments of observers in the local-constraint group of Experiment 1: pretest (Block 1), practice (Blocks 2 and 3), and posttest (Block 4). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

test on the differences between these correlations revealed that this between-groups difference was significant, $t(14) = 2.3$, $p < .05$.

Experiment 2

Experiment 1 demonstrated that observers do not come to rely on the more globally useful kinematic variables if the variables they start with allow accurate performance (and hence rewarding feedback) during practice. As in the experiments of Michaels and de Vries (1998), particular characteristics of the stimulus sets determined the variables on which observers came to rely. This observation raises a number of general issues about the observed process of perceptual learning: How durable are the observed changes in reliance? Does the learned reliance transfer to other tasks? Do these findings generalize to other sense modalities, or even to the guidance of action? Or can the process of convergence on the more useful variables be facilitated? This final issue was addressed in Experiments 2 and 3.

The preceding finding—that observers graduate to a specifying variable only if they practice in a task ecology in which other kinematic variables are not perfectly correlated with the to-be-perceived property—suggests, perhaps, that observers would converge on the use of a specifying variable more easily in task ecologies in which other candidate kinematic variables were not at all useful. In Experiment 2, we compared three training regimens to examine this possibility: *no-variation* practice, *zero-correlation* practice, and *random* practice. As in Experiment 1, identical pretests and posttests permitted assessment of the effects of training. During training with the no-variation set, exit-speed difference could not be used (successfully) because, in each collision, the balls had equal exit speeds. Moreover, reliance on scatter-angle difference would also yield poor mass judgments because the scatter angles of the balls always differed by 40° or by 100°. Hence, observers in the no-variation group who used exit-speed difference would make the same judgment on every trial, and those who used scatter-angle difference would make one of two judgments. Presumably, they would be inspired to change their strategy, which might lead them to discover a specifying variable.

In zero-correlation practice, a set of collisions was used in which exit-speed and scatter-angle differences had correlations of near zero with the simulated mass ratios. Hence, judgments based on these differences, or on a linear combination of them, would necessarily yield poor performance and correspondingly poor feedback. We hypothesized that the discouraging feedback might guide observers to reliance on a specifying variable. Random practice was included as a control condition. In this condition, the precollision velocities varied randomly, which resulted in moderate correlations between the to-be-perceived mass ratios and the candidate kinematic variables. The correlations among the kinematic variables that follow from the mass ratios and the precollision velocities used in the different sets of collisions are presented in Table 2.

Method

Twenty-four students from the University of Uppsala were assigned randomly to the three groups. The experiment consisted of two sessions of about 1.5 to 2 hr each, mostly carried out on consecutive days. The first sessions comprised an 80-trial pretest and the first two 88-trial practice blocks. The second session comprised a third 88-trial practice block and an 80-trial posttest. Ten mass ratios were simulated in the pretest and posttest; they differed by equal logarithmic steps from 1:4 to 4:1. During training, 11 mass ratios were simulated; they differed by equal logarithmic steps from 1:3 to 3:1. Different precollision velocities were used in the three training conditions and in the test phases. Appendix A describes how precollision velocities were calculated, and Appendix B presents the sets of collisions used.

Table 2
Correlations Between the Kinematic Variables in Experiment 2

Variable	1	2	3	4
Test phases				
1. Specifying invariant[a]	—	.55	.67	.84
2. Exit-speed difference		—	.06	.65
3. Scatter-angle difference			—	.79
4. Exit-speed and scatter-angle difference[b]				—
No-variation practice				
1. Specifying invariant[a]	—		.73	.73
2. Exit-speed difference		—		
3. Scatter-angle difference			—	1.00
4. Exit-speed and scatter-angle difference[b]				—
Zero-correlation practice				
1. Specifying invariant[a]	—	−.01	−.02	.03
2. Exit-speed difference		—	−.59	−.39
3. Scatter-angle difference			—	−.51
4. Exit-speed and scatter-angle difference[b]				—
Random practice				
1. Specifying invariant[a]	—	.81	.50	.91
2. Exit-speed difference		—	.11	.89
3. Scatter-angle difference			—	.55
4. Exit-speed and scatter-angle difference[b]				—

[a] Correlates perfectly with the simulated mass ratios. [b] We used the linear combinations of the differences in exit speed and scatter angle that best predicted the simulated mass ratios in the to-be-considered sets of collisions to calculate the correlations shown.

Results and Discussion

As in Experiment 1, we first assess whether observers were able to perceive relative mass and whether the judgments improved after practice. In the second set of analyses, we aim to reveal which kinematic variables were used. Figure 5 presents judgments averaged per simulated mass ratio and per group in the pretest (left panel) and posttest (right panel). All averaged judgments reasonably approximated the simulated mass ratios; participants seemed to be able to perceive relative mass. This conclusion was confirmed by the correlations between the judged and simulated mass ratios. The squares of these correlations are presented in Figures 6, 7, and 8. In the pretest and posttest, all but three of the correlations differed significantly ($p < .05$) from zero; for a majority of observers, variation in judgments corresponded to variation in simulated mass ratios.

We performed an ANOVA on the correlations between the judgments and the simulated mass ratios; group (no variation vs. zero correlation vs. random) was a between-subjects variable, and experimental phase (pretest vs. posttest) was a within-subject variable. The effect of phase was significant, $F(1, 21) = 4.9$, $p < .05$, indicating that the correlations were higher after practice. The effect of group, $F(2, 21) = 0.8$, $p > .10$, and the interaction, $F(2, 21) = 0.3$, $p > .10$, were not significant. Later we show that the lack of significant differences among the practice conditions might have been due to the large differences among observers.

To test which kinematic variables were used and whether observers differed and changed in the variables they used, we calculated, for each block of trials and each observer, correlations between the candidate variables and the judgments. For the no-variation group, the squares of these correlations are presented in Figure 6. There were large and interesting differences both between and within participants. The exception was Observer 5, whose judgments never correlated highly with any of the candidate variables; it is unclear whether this observer used some other variable or simply did not understand the task. The judgments of Observer 2 and, to a lesser extent, the judgments of Observers 4 and 8 correlated highly with the simulated mass ratios in all blocks; these observers seemed to rely on a specifying variable throughout the experiment.

Observers 1, 3, 6, and 7 are the most interesting; they seemed to start with reliance on exit-speed difference. During practice with feedback, in which the exit speeds of the balls were equal, Observers 3 and 6 changed their strategy and came to rely on a specifying variable. But they did not continue to do so in the posttest; when the variation in exit-speed difference resumed, so did their exploitation of it. Observer 1, whose judgments were not predicted by any of our candidate variables during practice, also returned to the use of exit-speed difference in the posttest. Finally, Observer 7 seemed to rely on a combination of the exit-speed and scatter-angle differences in the posttest. In sum, and as expected, observers who started with reliance on the nonspecifying variables changed their strategy during training. However, in the posttest,

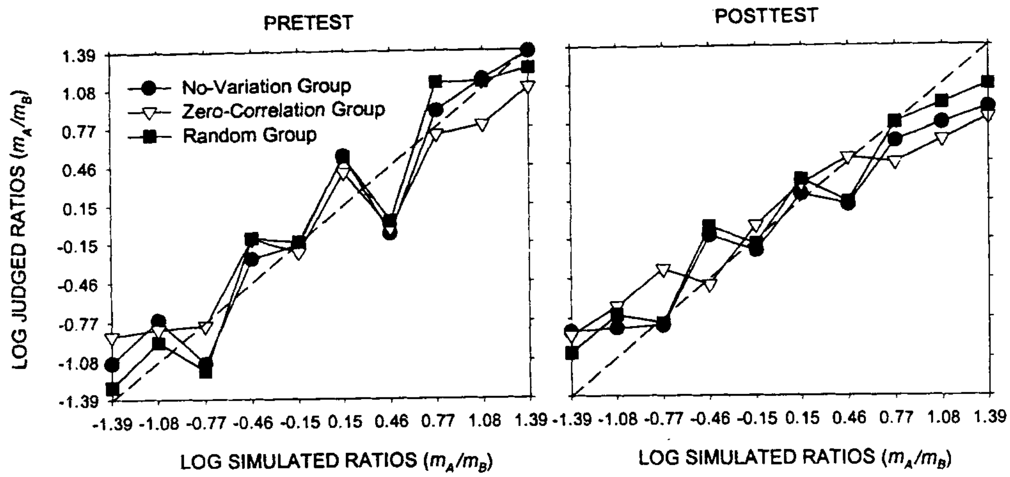

Figure 5. Averaged logarithms of judged mass ratios as a function of the logarithms of the simulated mass ratios in the pretest (left) and posttest (right) for the three groups in Experiment 2. The simulated mass of Ball A (the dotted circle) is referred to as m_A, and the simulated mass of Ball B (the continuous circle) is referred to as m_B.

most of these observers returned to the use of the nonspecifying variables.

The results of the zero-correlation group are presented in Figure 7. Most observers either learned to use a specifying variable or learned not to use a nonspecifying variable. The former case is illustrated by Observers 2, 3, 5, and 8, who seemed to rely on a specifying variable in the posttest but not in the pretest. Observers 4 and 7, in turn, seemed to learn not to use the candidate nonspecifying variables, although the variables that they did use in the posttest, if any, were not among our candidates. Observer 1 appeared to have learned not to use exit-speed difference but may have simply exchanged it for scatter-angle difference. Although the results are not entirely clear, Observer 6 might have used a combination or a specifying invariant in all blocks of trials.

In the random group (Figure 8), there appeared to be less change from block to block. Observers 1 and 2 seemed to rely on the combination of exit-speed and scatter-angle differences throughout the experiment. Observer 5 followed this pattern up until the posttest, in which he appeared to detect a specifying variable. For Observers 3, 4, 6, 7, and 8, it was difficult to distinguish which of the kinematic variables was used. Note the difference between this condition and the global-constraint condition in Experiment 1. In the present condition, the multiple correlations between the judgments and the exit-speed and scatter-angle differences often seemed to be based primarily on the exit-speed difference, whereas, in the global-constraint condition of Experiment 1, the scatter-angle differences seemed to contribute to the predictions of the judgments in almost all blocks of trials for all observers. This seems to reflect the relative usefulness of these variables; the correlations between exit-speed and scatter-angle differences and judgments were .81 and .50 here and .44 and .83 in the global-constraint condition of Experiment 1.

In short, Figures 6 to 8 suggest that the no-variation practice did not foster reliance on a specifying variable because, after practice, observers returned to using nonspecifying variables; the use of nonspecifying variables seemed to be suppressed rather than replaced. Observers in the zero-correlation practice learned not to use the nonspecifying variables. For some observers, this facilitated convergence on mass-specifying information, but observers who did not discover the more useful variables continued to deteriorate, perhaps because the kinematic variables they tried to use seemed to be useless. Finally, most observers in the random group performed reasonably accurately after practice because they came to rely on nonspecifying variables that correlated with the mass ratios in practice as well as in the test phases. However, these reasonable correlations seemed to decrease the chance that observers graduated to the use of a specifying variable.

We can summarize these findings by comparing the correlations between the judgments and relative mass with the multiple correlation between the judgments and exit-speed and scatter-angle differences. Figure 9 presents the differences between these correlations. If observers learned not to use the nonspecifying variables, this difference should increase over practice. In agreement with the conclusions from Figures 6–8, the difference seemed to increase over practice for the zero-correlation group, increase moderately for the random group, and stay constant or decrease for the no-variation group. We performed an ANOVA on these difference scores; group (no variation vs. zero correlation vs. random) was a between-subjects variable, and experimental phase (pretest vs. posttest) was a within-subject variable. The effect of phase was significant, $F(1, 21) = 8.4$, $p < .01$, suggesting that observers relied more on specifying information and less on nonspecifying variables in the posttest than in the pretest. The effect of group was not significant, $F(1, 21) = 2.0$, $p > .10$. Finally, the interaction was significant, $F(2, 21) = 4.9$, $p < .05$. This indicates, indeed, that the different practice conditions effected different changes in the tendency to use nonspecifying variables.

Experiment 3a

In Experiments 1 and 2, as well as in the experiments of Michaels and de Vries (1998) and Jacobs et al. (2000), observers differed from each other and changed in the optical variables on which they based their responses. This variability between and

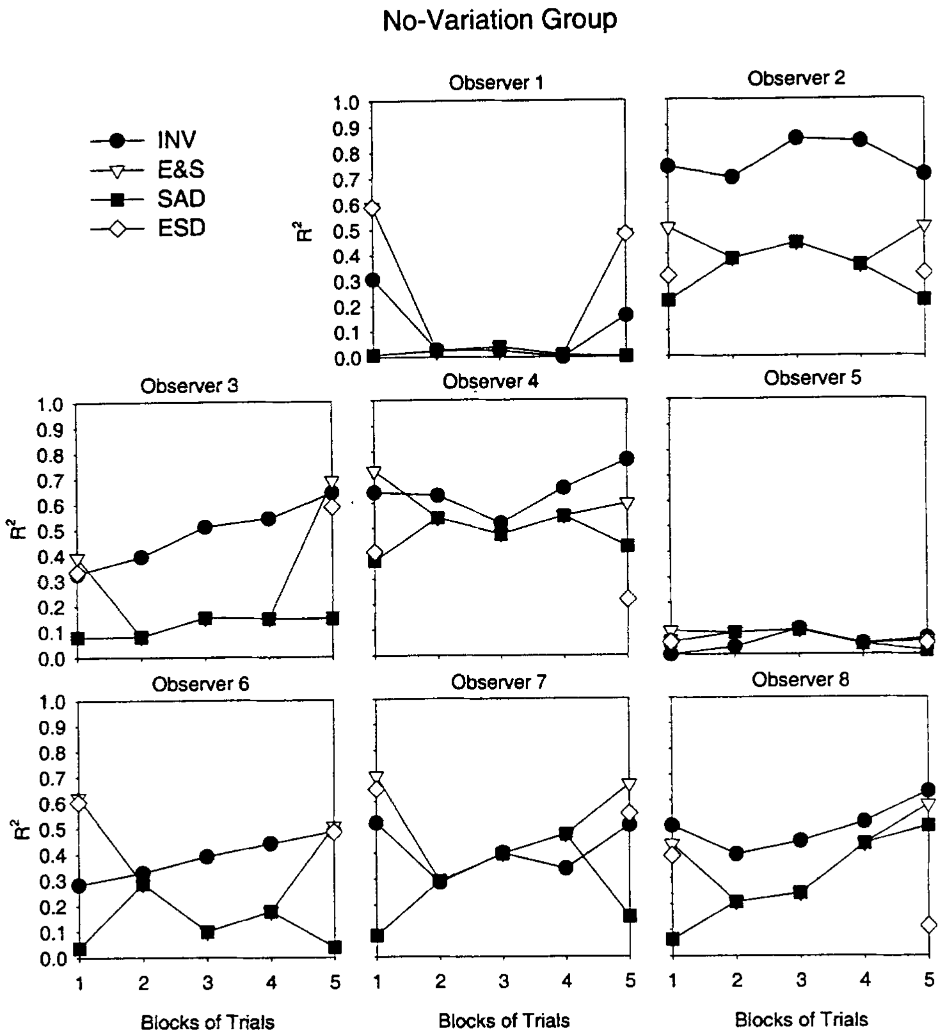

Figure 6. Squares of correlations between the various kinematic variables and judgments of observers in the no-variation group of Experiment 2: pretest (Block 1), practice (Blocks 2, 3, and 4), and posttest (Block 5). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

within observers is an important finding; it implies that it is a less interesting scientific question to ask which optical variables observers use in general or at a particular moment because these variables are determined by a variety of task characteristics and individual differences (Jacobs & Michaels, 2001). The to-be-discovered regularities may reside at the level of the process of convergence on the more useful variables rather than on the level of which optical variable is used at a particular moment. The use of different sets of practice displays appears to be helpful in investigating the learning process, but the large individual differences, although important in their own right, tend to obscure the effects of the experimental conditions.

Understanding the more general processes can involve ignoring some of the particulars of the results. For instance, despite the individual differences in variable use, observers in the zero-correlation practice of Experiment 2 learned not to rely on the nonspecifying variables. Some observers, though, did not appear to discover other variables that could lead to accurate performance and hence performed poorly on the posttest. This led to differences in performance much larger than those typically observed. It is illustrative in this regard to compare the posttest performance of observers in the zero-correlation and random conditions of Experiment 2 (Figures 7 and 8). Remember that the posttest was the same for both groups. The differences among observers in the random group were quite modest relative to the differences among observers in the zero-correlation group. Apart from reducing the tendency to rely on nonspecifying variables, the zero correlations increased the risk that some observers did not discover any useful variable and hence performed poorly on the posttest. This may have led to the larger differences among individuals.

One might expect that the differences among observers would be smaller than in the zero-correlation practice of Experiment 2 if only one of the nonspecifying variables did not correlate with relative mass in practice. In that case, fewer observers would be expected to perform poorly in the posttest because more variables would allow reasonably accurate performance. If so, the effect of

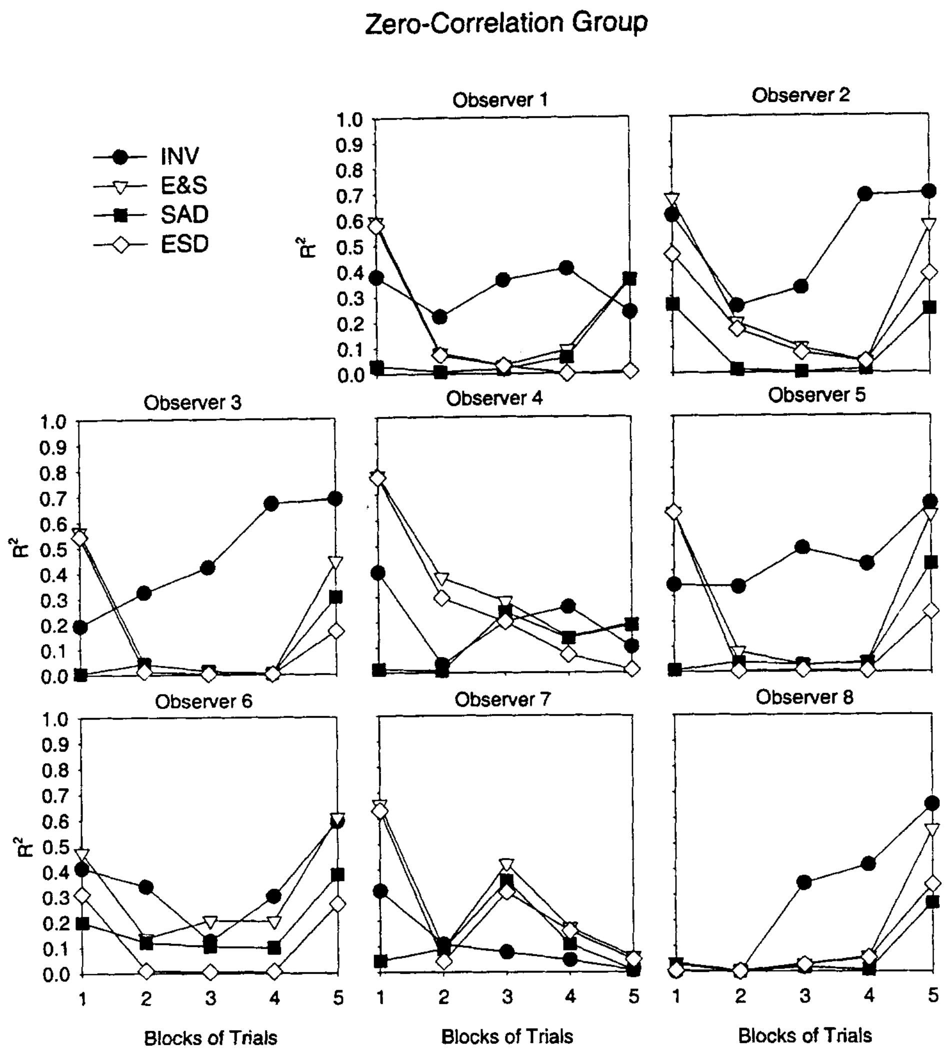

Figure 7. Squares of correlations between the various kinematic variables and judgments of observers in the zero-correlation group of Experiment 2: pretest (Block 1), practice (Blocks 2, 3, and 4), and posttest (Block 5). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

the zero correlation might be revealed more clearly. Two practice sets of collisions were used in Experiment 3 to test this possibility. In *speed–correlation zero practice,* exit-speed difference did not correlate with relative mass, but scatter-angle difference correlated normally (r = .75) with relative mass. In *angle–correlation zero practice,* scatter-angle difference did not correlate with relative mass, but exit-speed difference correlated normally (r = .75) with relative mass. As in Experiments 1 and 2, identical pretests and posttests were used to assess the effects of the different practice conditions. Table 3 presents the correlations between the candidate kinematic variables in the sets of collisions used.

We hypothesized that observers who initially use a nonspecifying variable that does not correlate with relative mass during practice learn not to rely on that variable and come to exploit a variable that is more useful (at least in practice). Alternatively, observers who start with reliance on a variable that correlates moderately with relative mass during practice might or might not change to a more useful variable. Table 4 presents the predictions about the variables observers use in the posttest that follow from this hypothesis, given the practice conditions and the variables observers use in the pretest. It is assumed that observers initially use exit-speed difference, a combination of the nonspecifying variables, or mass-specifying information but not scatter-angle difference, as was the case in the previous experiments.

Method

Sixteen students from the University of Uppsala were assigned randomly to the two groups. The experiment consisted of two sessions of about 1.5 to 2 hr each, mostly carried out on consecutive days. The first sessions comprised an 80-trial pretest and a first 88-trial practice block. The second session comprised the second and third 88-trial practice blocks and an 80-trial posttest. The various parameters of the collisions used in the different blocks are presented in Appendix B. The sets of collisions were calculated with an algorithm similar to the one used to calculate the set of collisions used in the zero-correlation practice of Experiment 2 (see Ap-

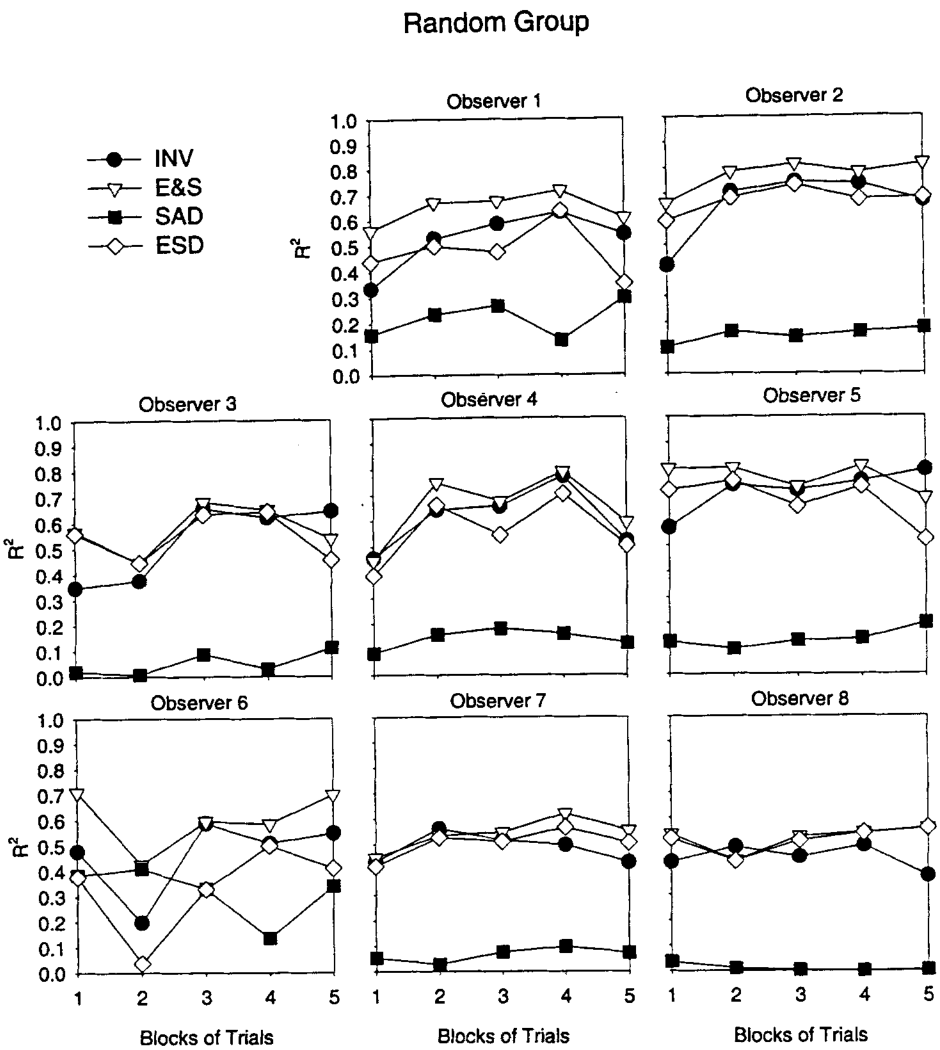

Figure 8. Squares of correlations between the various kinematic variables and judgments of observers in the random group of Experiment 2: pretest (Block 1), practice (Blocks 2, 3, and 4), and posttest (Block 5). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

pendix A). In all other regards, the experiment was the same as Experiment 2.

Results and Discussion

Figure 10 presents judgments averaged per simulated mass ratio and per group in the pretest (left panel) and posttest (right panel). All averaged judgments approximated the simulated ratios; observers appeared able to perceive relative mass, at least to some extent. As in Experiments 1 and 2, this conclusion was confirmed by the correlations between the judgments and relative mass. The squares of these correlations are presented in Figures 11 and 12. All of the 80 correlations differed significantly from zero ($p < .05$).

To determine which variables were used by individual observers, we computed the correlations between judgments and candidate kinematic variables. For the speed–correlation zero group, the squares of these correlations are presented in Figure 11. Recall that exit-speed difference did not correlate with relative mass in speed–correlation zero practice and that we expected observers who initially used this variable to learn not to use it (see Table 4). Observers 3, 4, 5, 7, and 8 relied on the exit-speed difference in the pretest. All of these observers changed in the variables on which they relied; Observers 3 and 8 appeared to detect mass-specifying information in the posttest, and Observers 4, 5, and 7 appeared to rely on the combination. Observers 1 and 6 relied on the combination in the pretest. In agreement with the predictions in Table 4, these observers appeared to use specifying information in the posttest. Only the performance of Observer 2 did not agree with the predictions in Table 4; this observer seemed to rely on specifying information in the pretest and on a combination in the posttest. In sum, observers in the speed–correlation zero group learned not to rely on the difference in exit speeds and discovered the more useful kinematic variables.

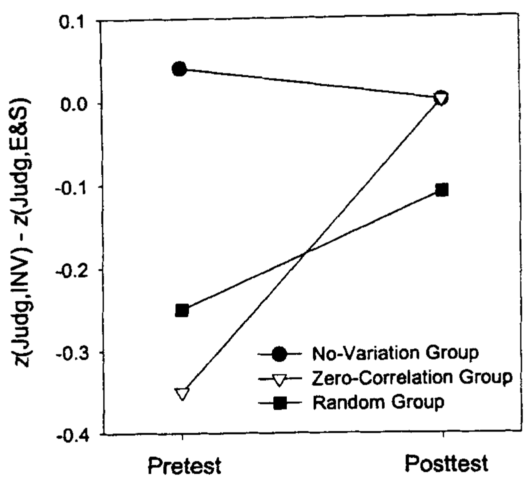

Figure 9. Averaged z scores of the correlations between judgments and a specifying invariant (Judg, INV) minus the z scores of the multiple correlations between the judgments and the differences in exit speed and scatter angle (Judg, E&S), for the three groups in Experiment 2 in the pretest and posttest.

The results of the angle–correlation zero group are presented in Figure 12. Again, most interesting are the observers who relied on the exit-speed difference in the pretest (Observers 2, 4, 5, 6, and 7). Use of exit-speed difference could lead to reasonably accurate performance during angle–correlation zero practice because the correlation between relative mass and exit-speed difference was .75. Observers 2, 4, and 6 and, to a lesser extent, Observer 5 continued to rely on exit-speed difference throughout the experiment. Observer 7 came to detect mass-specifying information in the posttest. Observers 1 and 8 seemed to change from reliance on a combination to reliance on a specifying variable. Finally, Observer 3 used specifying information in the pretest as well as in the posttest. Note that all of these changes in performance were in agreement with the predictions formulated in Table 4.

Table 3
Correlations Between the Kinematic Variables in Experiment 3

Variable	1	2	3	4
		Test phases		
1. Specifying invariant[a]	—	.50	.50	.78
2. Exit-speed difference		—	−.20	.63
3. Scatter-angle difference			—	.64
4. Exit-speed and scatter-angle difference[b]				—
		Speed-correlation zero practice		
1. Specifying invariant[a]	—	.00	.75	.81
2. Exit-speed difference		—	−.38	.00
3. Scatter-angle difference			—	.93
4. Exit-speed and scatter-angle difference[b]				—
		Angle–correlation zero practice		
1. Specifying invariant[a]	—	.75	.00	.77
2. Exit-speed difference		—	−.23	.97
3. Scatter-angle difference			—	.00
4. Exit-speed and scatter-angle difference[b]				—

[a] Correlates perfectly with the simulated mass ratios. [b] We used the linear combinations of the differences in exit speed and scatter angle that best predicted the simulated mass ratios in the to-be-considered sets of collisions to calculate the correlations shown.

Table 4
Predictions of the Kinematic Variables That Observers Would Use in the Posttest of Experiment 3a, Given the Variables They Used in the Pretest and Practice Conditions

Speed–correlation zero condition		Angle–correlation zero condition	
Variables observers might use in pretest	Variables observers are predicted to use in posttest	Variables observers might use in pretest	Variables observers are predicted to use in posttest
INV	INV	INV	INV
E&S	E&S or INV	E&S	E&S or INV
ESD	SAD, E&S, or INV	ESD	ESD, E&S, or INV

Note. INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; ESD = exit-speed difference; SAD = scatter-angle difference.

Together, Figures 11 and 12 suggest that observers learned not to rely on exit-speed difference after speed–correlation zero practice but did not do so after angle–correlation zero practice. These findings are summarized in Figure 13, which presents the difference between (a) the correlation of judgments with exit-speed difference and (b) the correlation of judgments with scatter-angle difference. In agreement with the conclusions from Figures 11 and 12, this difference decreased dramatically from the pretest to the posttest for participants in the speed–correlation zero group but not for participants in the angle–correlation zero group. A one-tailed t test revealed that the decrease was significantly larger in the speed–correlation zero group, $t(14) = 6.1$, $p < .001$; speed–correlation zero practice led to a larger reduction in the use of exit-speed difference than angle–correlation zero practice.

Consequently, one might predict that observers improve more after speed–correlation zero practice than after angle–correlation zero practice, because in the pretest exit-speed difference is used more frequently than scatter-angle difference. Indeed, the correlations between the judgments and relative mass seemed to increase more after the speed–correlation zero practice ($r_{\text{pretest mean}} = .59$, $r_{\text{posttest mean}} = .80$) than after the angle–correlation zero practice ($r_{\text{pretest mean}} = .60$, $r_{\text{posttest mean}} = .73$). However, a one-tailed t test showed that the difference in the increase was only marginally significant, $t(14) = 1.7$, $p = .052$.

Experiment 3b

Experiment 3a revealed large differences in the posttest judgments of participants in the speed–correlation zero and angle–correlation zero groups. A further demonstration of the effect of the reduced correlations in practice might be obtained through the use of a within-observer design. Unfortunately, several difficulties are associated with the use of within-observer designs in learning experiments such as these. For instance, because observers learned not to use exit-speed difference after the speed–correlation zero practice of Experiment 3a, we cannot test whether these same

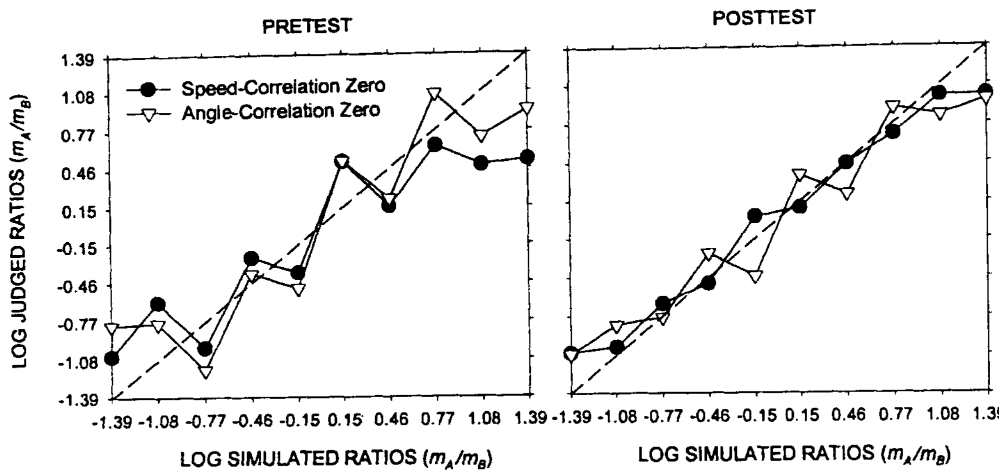

Figure 10. Averaged logarithms of judged mass ratios as a function of the logarithms of the simulated mass ratios in the pretest (left) and posttest (right) for both groups in Experiment 3a. The simulated mass of Ball A (the dotted circle) is referred to as m_A, and the simulated mass of Ball B (the continuous circle) is referred to as m_B.

observers might also have learned not to use this nonspecifying variable after the angle–correlation zero practice. Remember, though, that 4 observers in the angle–correlation zero condition (Observers 2, 4, 5, and 6) continued to rely on exit-speed difference throughout the experiment. The purpose of Experiment 3b was to test whether these observers would learn not to rely on exit-speed difference after speed–correlation zero practice.

Method

Observers 2, 4, 5, and 6 of the angle–correlation zero group of Experiment 3a participated in an additional session of about 2 hr. The session consisted of two practice blocks with feedback and a posttest without feedback. The practice blocks were identical to the speed–correlation zero blocks of Experiment 3a, and the posttest was identical to the test phases of Experiment 3a. The experiment took place between 2 and 6 days after the second session of Experiment 3a.

Results and Discussion

The squares of the correlations between the judgments and the candidate kinematic variables are presented in Figure 14, which also shows the correlations for these observers in the posttest of Experiment 3a. In contrast to angle–correlation zero practice, speed–correlation zero practice effected large changes in the judgments of Observers 2, 5, and 6. The performance of these observers differed remarkably during practice. Observer 2 continued to rely on exit-speed difference in the first practice block, Observer 5 simply exchanged exit-speed difference for scatter-angle difference, and Observer 6 immediately came to rely on the combination. In the posttest, though, all of these observers came to rely on the combination. This was not the case for Observer 4, who continued to rely on exit-speed difference, despite the feedback indicating poor performance.

In sum, these observers continued to rely on exit-speed difference after angle–correlation zero practice, but most of them learned not to rely on this nonspecifying variable after speed–correlation zero practice. To test whether this difference between the practice conditions was significant, we computed the difference between (a) the correlation of judgments with exit-speed difference and (b) the correlation of judgments with scatter-angle difference. On average, this difference increased after the angle–correlation zero practice (i.e., from the pretest of Experiment 3a to the posttest of Experiment 3a) but decreased after the speed–correlation zero practice (i.e., from the posttest of Experiment 3a to the posttest of Experiment 3b). A *t* test for paired samples indicated that this difference between the practice conditions was significant, $t(3) = 3.7$, $p < .05$.

Finally, we want to mention some spontaneous remarks made in the breaks and after the experiment. Observer 5 reported that the speed–correlation zero practice consisted of a different type of collisions than the angle–correlation zero practice. When we asked whether he could further explain this, he could not, and neither did he know whether or not the collisions in the test phases were of a different type. Observer 2 reported that she was very confused because the new collisions appeared to be similar, but at the same time they were more difficult. Observer 4 apologized for performing poorly; she reported that she tried as hard as before and that she did not know why she performed so poorly.

General Discussion

Participants in the present experiments were trained to judge the relative mass of colliding balls with sets of collisions in which the family of relations between the candidate kinematic variables and the mass ratios differed. We investigated how the different relations affect convergence on the more useful variables, as demonstrated in earlier studies (e.g., Michaels & de Vries, 1998). In Experiment 1, two learning conditions were compared. Participants in the local-constraint group were trained with a set of collisions in which all candidate kinematic variables correlated perfectly with the mass ratios and hence allowed accurate performance. In such a situation, observers did not appear to change in the variables they used. Participants in the global-constraint group were trained with a set of collisions in which the correlations between nonspecifying variables and to-be-judged mass ratios were lower (see Table 1 for the precise values). Like participants

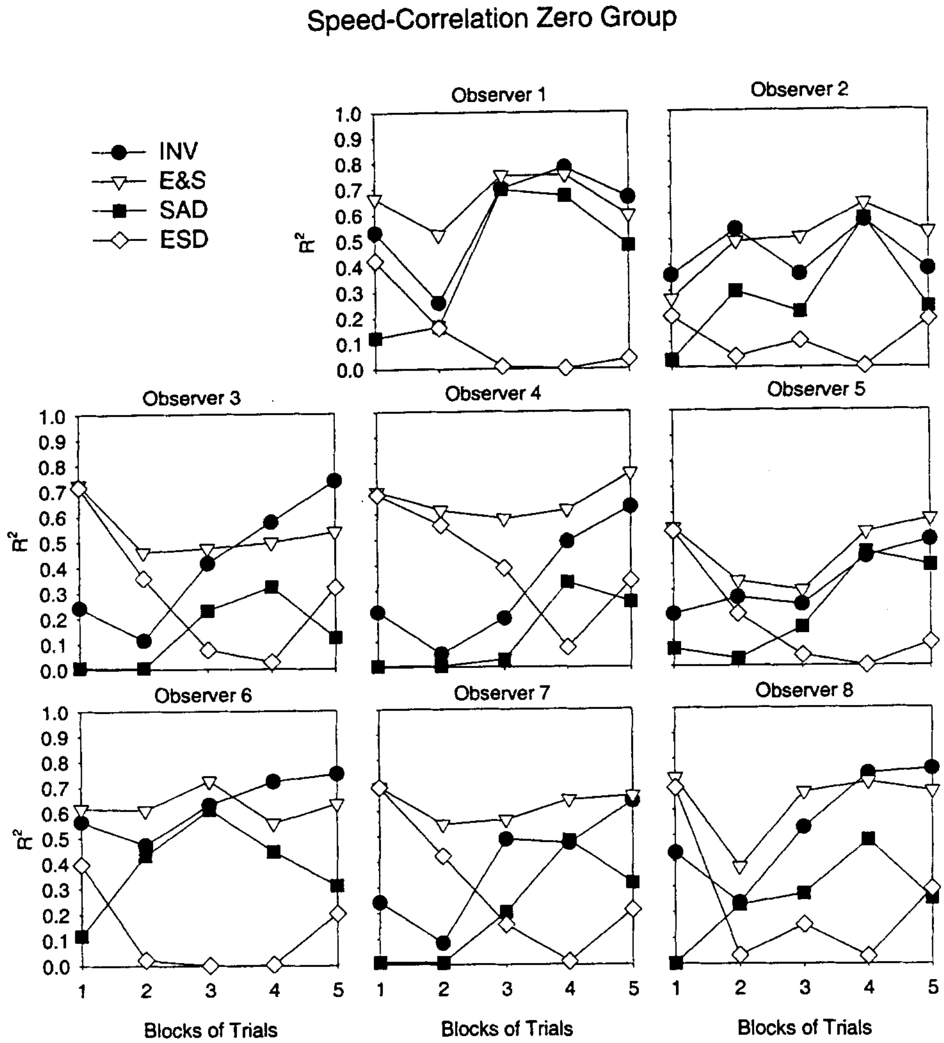

Figure 11. Squares of correlations between the various kinematic variables and judgments of observers in the speed–correlation zero group of Experiment 3a: pretest (Block 1), practice (Blocks 2, 3, and 4), and posttest (Block 5). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

in previous experiments, these observers did change in the variables they used and converged on the more useful ones.

The purpose of Experiment 2 was to test whether convergence on a specifying variable could be facilitated by sets of collisions in which commonly used, nonspecifying variables are rendered useless. The experiment included three practice conditions. In the no-variation condition, the candidate nonspecifying variables were rendered useless by holding them constant. Some observers indeed changed their strategy and appeared to detect a specifying variable during no-variation practice, but they fell back on their old strategies in a posttest in which the nonspecifying variables varied again. In the zero-correlation condition, the candidate nonspecifying variables varied, but were not systematically related to the mass ratios. As in the no-variation group, these participants abandoned their nonspecifying variable during practice; unlike the no-variation group, however, they did not revert to their old variable during the posttest. Almost all observers learned not to use the candidate nonspecifying variables after the zero-correlation practice, and at least some of them discovered a specifying variable. Finally, in the random practice condition, the nonspecifying variables correlated moderately with the mass ratios (see Table 2 for the precise values). All observers in the random group relied on the more useful nonspecifying variables, and few of them appeared to discover a specifying variable.

Experiment 3a was a variation of the zero-correlation condition of Experiment 2; observers practiced with sets of collisions wherein a single nonspecifying variable was not systematically related to relative mass. Learning was primarily limited to observers who started with the variable rendered useless during practice. Experiment 3b further demonstrated the effect of practice with reduced correlations in a within-observer design.

In all experiments, average performance—as measured by the correlations between judgments and mass ratios—improved after practice with feedback. In Experiment 1, observers performed

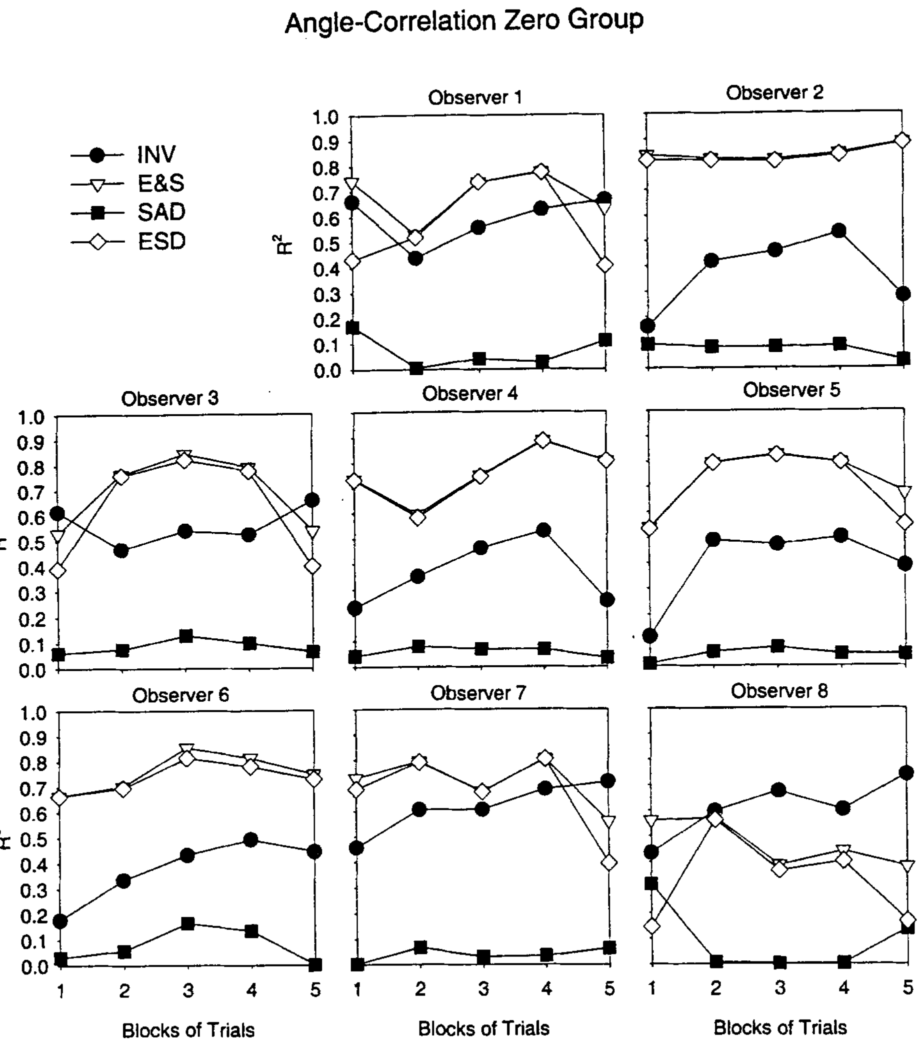

Figure 12. Squares of correlations between the various kinematic variables and judgments of participants in the angle–correlation zero group of Experiment 3a: pretest (Block 1), practice (Blocks 2, 3, and 4), and posttest (Block 5). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

better after global-constraint practice than after local-constraint practice. Training is more successful if commonly used nonspecifying variables correlate less than perfectly with the quantity to be perceived.

In Experiment 2, we did not find significant differences in overall levels of performance after the different practice conditions. Nevertheless, the results of the experiment might have implications for the design of optimal practice conditions. Training with moderate correlations between the to-be-perceived quantity and commonly used nonspecifying variables (as in the global-constraint group of Experiment 1 and the random group in Experiment 2) seems to be appropriate if all participants in a practice group need to achieve a certain level of performance. If, on the other hand, the purpose of practice is to reach high levels of performance with a few participants, one could use zero-correlation practice and then select the best performing observers. After zero-correlation practice, most observers learn not to use the candidate nonspecifying variables; they might or might not discover a specifying variable to use instead. Those observers who discover a specifying variable improve dramatically and reach high levels of performance.

Experiment 3 seems to indicate that practice is most effective if the nonspecifying variable on which observers rely initially is the only nonspecifying variable that does not correlate with the property to be perceived. This means that the design of optimal practice conditions would benefit from knowledge about the optical variables that novices tend to use. It also seems to imply that different practice conditions are optimal for different observers in situations in which novices differ in the optical variables they use.

Our results are in agreement with the thesis from direct perception theory that perception is specific to optical variables (Gibson, 1966, 1979; Michaels & Carello, 1981). The observed differences and changes in variable use run counter, however, to the hypothesis that observers always rely on the same optical variable in a particular task. This hypothesis underlies many theoretical and empirical studies in the fields of visual perception and visually

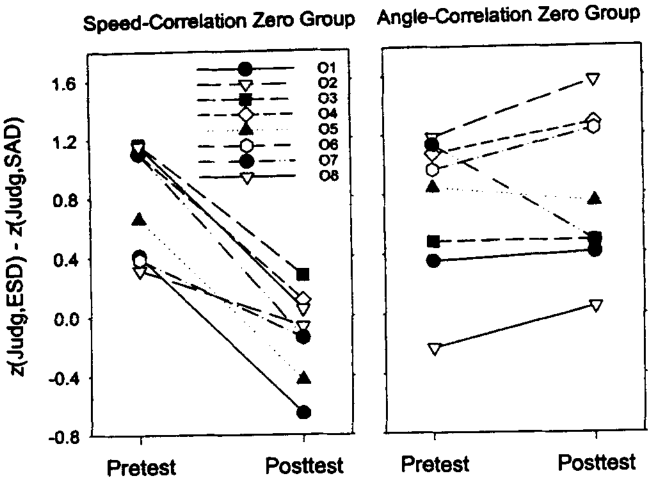

Figure 13. Averaged z scores of the correlations between judgments and exit-speed difference (Judg, ESD) minus the z scores of the correlations between judgments and scatter-angle difference (Judg, SAD) in the pretest and posttest, for each participant in the speed–correlation zero group (left) and the angle–correlation zero group (right) of Experiment 3a.

guided action. For instance, the timing of interceptive action is often said to be geared to the optical variable τ (e.g., Lee, 1976; Lee & Reddish, 1981; Lee, Young, Reddish, Lough, & Clayton, 1983; Savelsbergh, Whiting, & Bootsma, 1991). By and large, these studies claim that τ is used for the timing of all interceptive acts, by all observers, and at all levels of expertise, a claim that has been referred to as the "τ hypothesis" (Tresilian, 1999). However, the empirical support for the τ hypothesis has been criticized (Tresilian, 1993; Wann, 1996), and more recent evidence suggests that other variables can also be used in the guidance of interceptive actions (Bennett, Van der Kamp, Savelsbergh, & Davids, 1999; Michaels, Zeinstra, & Oudejans, 2001; Tresilian, 1999; Van der Kamp, Bennett, Savelsbergh, & Davids, 1999; Van der Kamp, Savelsbergh, & Smeets, 1997).

One possible reason for the variety in variable use is that judgments and simple actions do not have to be very precise. With stricter task demands, one might observe fewer differences and changes in the variables that observers use. However, despite the considerable scientific effort, the single optical variable used for interceptive timing remains unknown. Maybe there is no such variable. Participants in our experiments flexibly changed in the variable they used and converged on the more useful ones. In our view, this implies that one should search for lawfulness not at the level of which variables are used but at the level of the process of convergence. What is needed is a theory that further addresses when and why observers change in the variables they use.

The importance of a well-articulated theory of perceptual learning for direct perception theory was emphasized by Michaels and Beek (1995), who, at the same time, criticized the existing literature on that issue, among other things, for its meager empirical base (see also Eppler & Adolph, 1996, for a commentary). Our findings should contribute to that empirical base. During practice, observers appear to search for detectable variables that predict the quantity to be perceived; perception improves because observers come to rely on the more useful variables. Eventually, it seems, observers can even discover variables that specify to-be-perceived properties of the environment.

We conclude with some remarks on specificity. It is important to note that optical variables can be specific to environmental properties only by virtue of constraints on the ecology to be considered. Constraints are the necessary *grantors of information* (Runeson, 1988, 1989). The law of conservation of linear momentum, for instance, grants a one-to-one relation between the relative amount of velocity change and the mass ratio of colliding balls (Runeson, 1977/1983). Similarly, the law of conservation of linear momentum and a lack of variation in precollision velocities grant a one-to-one relation between the exit-speed difference and mass ratio of the balls. Thus, whether a particular optical variable specifies a particular environmental property depends on the constraints on the considered ecology.

We have used the term *specifying variables* to refer to variables that are specific to relative mass by virtue of global constraints. However, in more restricted collections of displays, such as the one used in the local-constraint practice of Experiment 1, otherwise nonspecifying variables might also be specific to mass ratio. Specifying relations granted by global constraints (e.g., natural laws) might seem more useful for perceiver-actors than those granted by local constraints (e.g., certain precollision velocities), because they allow accurate performance in a wider range of task

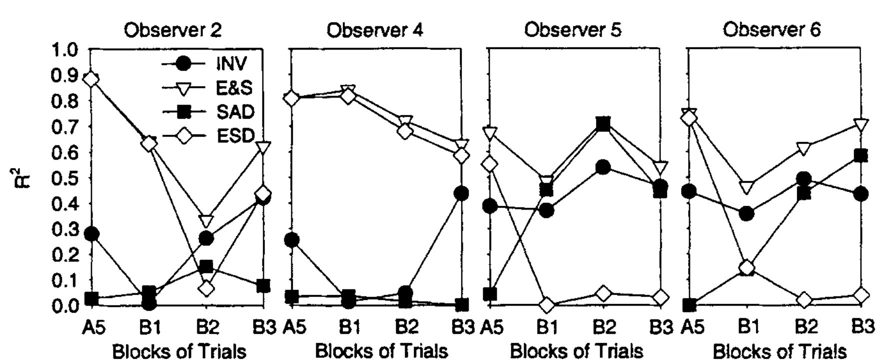

Figure 14. Squares of correlations between the various kinematic variables and judgments in the posttest of Experiment 3a (Block A5), the practice blocks of Experiment 3b (Blocks B1 and B2), and the posttest of Experiment 3b (Block B3). INV = specifying invariant; E&S = combination of exit-speed and scatter-angle differences; SAD = scatter-angle difference; ESD = exit-speed difference.

ecologies. One might expect observers always to take advantage of the existence of global constraints.[5] However, the present results suggest that observers merely search for variables that are useful in the ecology encountered in practice.

The variables observers come to detect after practice often appear to be the more useful nonspecifying variables. Whether observers ultimately move on to the use of specifying variables seems to depend on particular characteristics of the stimulus set. To the extent that nonspecifying variables happen to correlate highly with the property to be perceived, perceivers seem to become trapped in a local minimum. Thus, great care must be taken in the selection of a stimulus set; otherwise, what may appear to be global cognitive principles can, in fact, be local solutions to local problems.

[5] This expectation has been formulated by Gibson (1950): "In the course of the evolution of human vision, we might conjecture, all the existing variations within the retinal image have been utilized as stimuli for perception if they are consistently in correspondence with the actual lay of the land" (p. 114). Note, though, that Gibson (1966) also appeared to advocate the opposite view: "The information registered about objects and events becomes only what is needed, not all that could be obtained . . . only the information required to identify a thing economically tends to be picked up" (p. 286).

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(Appendixes follow)

Appendix A

Composition of Sets of Collisions Used in Experiment 2

No-variation practice, zero-correlation practice, and random practice consisted of different sets of 22 collisions, which are presented in Appendix B. In the 88-trial practice blocks, these 22 collisions were presented four times, twice with two positive sweep components and twice with one positive and one negative sweep component (see Figure 1). The sweep components were reversed to increase the variety in collisions; the reversal had no effect on the relation between the mass ratios and the candidate kinematic variables. In the following, we describe how the different sets of 22 collisions with positive sweep components were created. Remember that, given the restitution factor, a collision is determined by the mass ratio and precollision velocities of the balls. The precollision velocities, in turn, are determined by two sweep components and a mean collision component (in the present experiment, the collision components always differed by 51 mm/s).

To create the 22 collisions used in the no-variation practice, we started, for each of the 11 mass ratios, with 150 mean collision components ranging in equal steps from 0 to 38.3 mm/s and, for the ball with the highest collision component, 200 sweep components ranging in equal steps from 2.6 to 51.0 mm/s. For each of the 330,000 combinations (11 mass ratios, 150 mean collision components, and 200 sweeps), we calculated whether there existed a sweep component of the second ball for which the exit speeds would be equal and, if so, what that value was. We then calculated the corresponding scatter angles and chose a set of 22 collisions (2 for each mass ratio) for which the scatter-angle differences were either 40° or 100°. (With the present ranges of mean collision components and sweep components, it is not possible to choose 2 collisions per mass ratio so that all scatter-angle differences are equal.)

To create the zero-correlation set of collisions, we chose 22 mean collision components and 44 sweep components randomly from the same ranges as in the no-variation condition. These velocity components were assigned randomly to the 11 mass ratios to form 22 collisions (2 for each mass ratio). The mean collision components and the sweep components were then repeatedly exchanged between the different collisions as long as this decreased the difference between the required correlations and actual correlations between the mass ratios and the differences in exit speed and scatter angle. Adjusting the mean collision components and sweep components with gradually smaller steps further optimized the correlations. The algorithm often ended in a local minimum, but it was repeated until a set of collisions was found in which the actual correlations differed only marginally from the required correlations.

In the set of collisions used in the random condition, the same mean collision components and sweep components were used as in the no-variation condition. However, a different set of collisions was created by randomly reassigning these components to the different mass ratios. To create the set of collisions used in the test phases, we randomly assigned two series of 10 mean collision components (ranging in equal steps from 0 to 38.3 mm/s) and four series of 10 sweep components (ranging in equal steps from 2.6 to 51.0 mm/s) to the 10 mass ratios to form 20 collisions (2 for each mass ratio). Including 20 collisions with opposite sweep components and using each collision twice formed a set of 80 collisions.

Appendix B

Sets of Collisions Used in Experiments 2 and 3

Table B1

Mass Ratios (Ball A/Ball B), Mean Collision Components (M_{cc}), and Sweep Components (sc_A and sc_B) in Test Phases of Experiments 2 and 3

	Experiment 2			Experiment 3		
Ratio	M_{cc}	sc_A	sc_B	M_{cc}	sc_A	sc_B
4.00	21.2	7.9	29.6	4.6	34.5	36.2
2.94	38.2	34.9	7.9	26.0	26.3	14.3
2.16	29.8	13.3	34.9	17.6	23.0	34.7
1.59	0.0	2.5	24.0	10.0	9.2	19.9
1.17	8.4	29.6	45.6	1.0	6.4	51.0
0.86	4.3	40.3	2.5	16.1	26.8	22.7
0.63	33.9	18.6	51.0	21.2	37.8	34.7
0.46	17.1	24.0	13.3	12.5	26.8	14.3
0.34	12.7	45.6	40.3	12.3	45.9	3.9
0.25	25.5	51.0	18.6	12.5	37.8	6.1
4.00	33.9	18.6	40.3	33.2	48.7	13.5
2.94	0.0	45.6	24.0	9.5	39.3	46.2
2.16	8.4	29.6	34.9	13.8	26.0	32.7
1.59	17.1	40.3	7.9	7.7	49.8	10.7
1.17	38.2	2.5	13.3	17.9	10.5	14.6
0.86	21.2	24.0	51.0	6.7	28.8	6.7
0.63	25.5	7.9	29.6	21.7	7.2	35.5
0.46	4.3	51.0	18.6	17.1	33.7	11.8
0.34	29.8	34.9	2.5	37.3	13.0	41.8
0.25	12.7	13.3	45.6	32.9	23.7	47.7

Note. Balls A and B represent, respectively, the balls with the higher and lower collision component. All velocity components are in millimeters per second. The 80-trial test blocks were composed of two presentations of these collisions and two presentations of collisions with similar values except that the sweep components of Ball B were negative.

Table B2

Mass Ratios (Ball A/Ball B), Mean Collision Components (M_{cc}), and Sweep Components (sc_A and sc_B) in Practice Blocks of Experiment 2

	No variation			Zero correlation			Random		
Ratio	M_{cc}	sc_A	sc_B	M_{cc}	sc_A	sc_B	M_{cc}	sc_A	sc_B
3.00	0.8	51.0	17.4	36.7	50.5	5.9	33.2	35.7	8.2
2.41	5.9	50.8	14.1	34.5	51.0	10.0	6.7	49.2	33.7
1.93	8.2	47.7	11.0	0.0	46.7	41.3	8.2	39.3	6.4
1.55	3.6	49.2	33.7	0.5	3.1	44.4	38.3	41.1	14.1
1.25	6.7	41.1	24.3	0.0	3.1	42.6	3.6	44.7	17.4
1.00	18.6	44.7	16.9	35.0	2.6	51.0	5.9	29.4	20.7
0.83	21.2	39.3	9.7	18.4	25.5	3.3	0.8	47.7	9.7
0.63	19.4	29.4	6.4	38.3	5.1	39.6	35.7	50.7	11.0
0.53	35.7	43.4	4.6	37.8	6.7	51.0	18.6	51.0	4.6
0.42	33.2	35.7	8.2	37.8	18.9	51.0	19.4	41.3	16.9
0.33	38.3	41.3	20.7	18.9	42.4	2.6	21.2	43.4	24.3
3.00	0.0	50.3	17.4	38.3	50.8	2.6	13.8	21.4	2.8
2.41	0.3	45.4	14.1	37.5	48.7	2.8	0.3	28.6	17.4
1.93	0.0	39.0	10.0	12.5	43.6	32.7	0.5	23.5	24.5
1.55	0.3	44.7	31.6	0.0	2.6	46.9	0.0	30.9	5.9
1.25	0.5	28.6	18.1	2.1	2.6	49.8	22.7	45.4	22.7
1.00	13.8	35.7	2.8	5.1	51.0	2.6	36.0	35.7	10.0
0.83	14.3	30.9	11.0	34.7	9.7	26.5	0.3	39.0	31.6
0.63	16.3	24.0	5.9	37.3	4.6	37.8	0.0	44.7	11.0
0.53	14.8	21.4	22.7	38.3	13.0	34.2	14.3	41.1	18.1
0.42	22.7	23.5	17.4	18.6	41.1	2.8	16.3	50.3	14.1
0.33	36.0	41.1	24.5	37.8	24.0	50.5	14.8	24.0	17.4

Note. The 88-trial practice blocks were composed of two presentations of these collisions and two presentations of collisions with similar values except that the sweep components of Ball B were negative.

(*Appendix continues*)

Table B3
Mass Ratios (Ball A/Ball B), Mean Collision Components (M_{cc}), and Sweep Components (sc_A and sc_B) in Practice Blocks of Experiment 3

	Speed–correlation zero			Angle–correlation zero		
Ratio	M_{cc}	sc_A	sc_B	M_{cc}	sc_A	sc_B
3.00	2.8	45.9	35.0	37.5	9.0	11.0
2.41	2.1	26.8	20.4	35.0	28.3	46.4
1.93	24.5	28.1	32.9	35.7	15.8	14.1
1.55	19.2	28.8	13.5	27.3	13.3	25.3
1.25	17.4	23.2	20.4	6.9	2.6	50.0
1.00	23.7	28.6	15.1	21.4	20.9	19.2
0.83	6.9	2.8	50.8	19.7	42.6	21.2
0.63	32.7	13.5	28.8	10.5	34.7	6.4
0.53	29.4	9.2	36.7	35.2	18.6	22.7
0.42	18.9	17.1	43.9	21.7	40.8	23.2
0.33	36.7	19.9	49.0	15.6	38.3	2.6
3.00	30.4	50.8	16.9	28.8	9.5	40.8
2.41	17.9	32.2	28.9	28.1	10.5	49.8
1.93	31.9	27.6	4.4	38.3	37.3	24.3
1.55	4.1	32.4	9.0	23.5	22.5	43.6
1.25	4.9	13.0	40.6	2.6	3.1	51.0
1.00	13.5	50.8	10.2	21.7	25.8	38.3
0.83	20.4	40.8	43.6	15.1	14.6	49.8
0.63	15.3	7.7	43.1	5.9	16.3	6.7
0.53	27.0	3.9	45.4	7.4	14.3	11.0
0.42	30.1	47.2	31.1	11.0	48.0	2.8
0.33	36.0	39.3	30.1	14.8	39.0	6.1

Note. The 88-trial practice blocks were composed of two presentations of these collisions and two presentations of collisions with similar values except that the sweep components of Ball B were negative.

Received September 30, 1999
Revision received June 6, 2000
Accepted October 25, 2000 ■